Reviewer's report

Title: Determinants of nutritional status among infants and young children in Somali Region, Ethiopia: a cross-sectional study.

Version: 1 Date: 9 June 2015

Reviewer: Giorgio Bedogni

Reviewer's report:

GENERAL COMMENT

I was asked to review this paper as BMC statistical referee. I was blinded to the reviews made by the other Reviewers.

MAJOR COMMENTS

1. Abstract and elsewhere: please, report 95% confidence intervals (95% CI) for prevalence estimates.

2. Abstract and *elsewhere*: please, consider *and discuss* that some predictors are very imprecise, e.g. the adjusted odds ratio of wasting is 0.38 (95% CI 0.14 to 0.99) for breastfed vs. not-breastfed infants. This corresponds to a reduction of the odds anywhere between 1% and 86%. See also comments below.

3. Abstract and elsewhere: please, spell out the acronyms AOR (adjusted odds ratio) and COR (crude odds ratio). Also, explain the "correction" underlying CORs in the abstract (but see comment on selection of predictors below).

4. Study design and sampling: what was the number of children potentially available for the study (N)? Which fraction of that number was sampled (n = 214; % of N)? Why? Please, explain how the size of the kebeles was taken into account by sampling (n = n1 + n2 +n3?).

5. Statistical analysis: the selection of covariates for multivariable analysis on the basis of their statistical significance at univariable analysis has many problems. I suggest to include in multivariable analysis also the variables that are already known to be risk factors, *independently* of how they performed at univariable analysis, e.g. https://books.google.it/books?id=bRoxQBIZRd4C&printsec=frontcover&dq=hosmer+lemeshow+2013&hl=en&sa=X&ei=FuF2VcioJ4GvsQG-nqPgCw&redir_esc=y#v=onepage&q&f=false.

6. Results: I would argue that the fact that bottle feeding had no "statistically significant effect" on the risk of wasting is irrelevant because the 95% CI of this effect (0.98 to 4.22) is compatible with anything from a 2% reduction to a 4-fold increase of the risk. Please, see major comments above on wide CIs and selection of predictors.

7. Results: please, avoid focusing on factors "significantly associated" (e.g. Page 9 para 3) with the outcome of interest. Consider instead the precision of the
effect sizes as evaluated by the 95%CI of the odds ratios.

8. Tables: please, add a column with the number of subjects with the given risk factor; remove the reference group (make it clear that it is "high" for the wealth index); explain the COR and AOR acronyms.

9. General: please, give the operational definition of breastfeeding and bottle feeding (from the "yesterday" of Table 4 one infers that bottle feeding was ascertained from the 24 hour recall. Is this so?).

MINOR COMMENTS

1. Title and elsewhere: "determinants" suggest a cause-effect relationship. I suggest to use "risk factors" instead.

2. Abstract: for coherency with the "multivariable" terminology you should use "univariable" to define "bivariate" analysis, e.g.

3. Abstract: you can delete the phrase about statistical significance set at a p-value < 0.05. See instead the major comment on confidence intervals.

4. Keywords: spell out IYCF -> infant and young child feeding?

6. Background: I suggest to specify that this applies to Africa (reference 1).

7. Background: the of prevalence of wasting in the DHES study is reported to be 22.8%. However, it is said to be 17.8 in the discussion%. I suppose that this is because the first is a general estimate and the second an age-specific estimate. Is this so? Please, explain.

8. Study design: Please report if validation data are available for the modified DHS questionnaire.

9. Study design: how may operators were involved in the collection of food data? Were they experienced operators?

10. Results: dyads -> this is the first occurrence of the term. Please, explain what it refers to.

11. Figure 4: I suggest to remove it as it does not add anything to the text.

12. Results: uni- and multi-variable analyses are discussed for wasting while only multi-variable analysis is reported for stunting and underweight. Why?

13. Results: EDHS -> please spell it out the first time you quote the Ethiopian DHS.

14. Results: "The absence of association..." (last phrase in para 1): it is not clear to me what are you trying to say here.

15. Table 1 I would report the mean (SD) number of relatives in the text. (I
suppose that it was normally distributed. If it is not, it would be better to report the median.)

DISCRETIONARY REVISIONS

1. A figure plotting the odds ratios for each uni- and multi-variable regression model side by side would considerably improve the understanding of the effects, e.g. ftp://repec.sowi.unibe.ch/files/wp1/jann-2013-coefplot.pdf (this is Stata-centric but the concept applies in general).

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests